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ORIGINAL COMMUNICATIONS.

THE PATHOLOGY OF CHOLERA INFANTUM.

BY DR. A. NEBINGER.

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WE need not say that, even at this late day, the nature and the primary lesions of cholera infantum are matters of doubt, therefore of discussion by writers and teachers of nosology and by practitioners of medicine. There are those who contend that cholera infantum is primarily a gastro-enteric malady *per se*, others, that it is a hepatic gastro-enteric disease,—the liver being the organ originally involved, and the gastro-enteric morbid phenomena and lesions the results or secondary effects of the hepatic disturbance. The question being an open one, we propose examining it and making some remarks upon what we consider the nature of the disease and its primary seat of morbific influence.

The anatomical lesions in cholera infantum which present themselves upon a post-mortem examination are such as indicate derangement of the liver, stomach, and bowels. The portal vessels have been found largely congested, and some investigators have reported cases where an enlargement of the liver existed. The gall-bladder has been found containing a large amount of thick, acrid, viscid bile. In some cases the gall-bladder has been found quite empty. Not a single case, as far as we are aware, has been reported by an undoubted investigator, where the gall-bladder was found containing normal bile. The gastric mucous membrane has been found inflamed, ulcerated, softened, and its follicles enlarged. The mucous membrane of the small intestines has revealed inflammation and ulceration; the glands, solitary and acuminate, enlarged; the mucous coat of the colon has evidenced inflammatory softening. Thus, it is seen, we have cholera infantum revealing, post-mortem, extensive derangements of the liver, stomach, and bowels. Here then let the question be asked, is cholera infantum primarily a disease of all the organs and tissues named, or has it its origin in one only? Does it at once present itself as a hepatic gastro-enteric disease, or is it primarily confined to one of these organs or parts, and gradually or rapidly, as may be, spread itself from one to another, until the whole train or series of the morbid conditions which exist in cholera infantum are produced? These questions have been variously, and in some instances curiously, answered by high authorities. Dr. George B. Wood says, "Cholera infantum may be safely inferred to consist essentially in an irritation or inflammation of the alimentary mucous membrane, directed especially to the mucous follicles, and associated with a congested and torpid state of the liver, probably depending upon the same cause."*

Dr. Robley Dunglison, in his article upon cholera infantum (The Cyclopædia of Pract. Med., 1850, vol. i. p. 427), observes, "that the liver is almost always enlarged, and that this has been regarded as the primary affection." Dr. J. F. Meigs, Pract. Treatise on Diseases of Children, 1848, p. 297, says, "I am disposed to believe that cholera infantum is a disease of the mucous membrane of the alimentary canal, which, beginning with morbid development of the mucous follicles or crypts, independent of evident inflammation, occasions first super-secretion from these organs, and after a time runs into inflammation and its results, ulceration, softening, and thickening." Dr. Condie, Diseases of Children, 1850, p. 234, remarks in reference to cholera infantum, "that comparing the symptoms during the life of the patient with the appearances discovered after death, it would appear to depend in its earlier stages upon hyperæmia of the mucous membrane, with an augmentation of the functions of the muciparous follicles of the alimentary canal." Dr. Dewees regarded "the congestion of the portal vessels and the enlargement of the liver as secondary effects."

It is unnecessary to further quote the various, somewhat discordant, and at best indefinite opinions entertained and put forth by authorities in regard to the nature, primary seat, or starting-point of action of the morbific influences in this very interesting disease.

Before advancing further, we take opportunity to remark that we do not regard the gastro-enteric phenomena of cholera infantum as primary, but secondary conditions, hence that neither the stomach nor the bowels are originally involved.

Observation, reflection, and the results of the treatment to which we have submitted cholera infantum patients, have brought us to the conclusion that exalted solar heat, long continued, acting upon and through the nervous system, upon the liver, is the cause, and that organ the primary seat of disease, from which radiate all the terrible morbific action, etc., which make up the *tout-ensemble* of cholera infantum. Follow, if you please, while we endeavor to present some of the facts, and construct the argument upon which, legitimately, we think, is based the conclusion that the liver is the primary seat of disease in cholera infantum.

1. The portal vessels are largely congested, the liver sometimes enlarged, the gall-bladder contains in some instances a dark, acrid, viscid secretion, in other instances it has been found nearly or quite empty, but the bile when found has always been abnormal.

2. The season when the disease occurs is summer, or in other words in hot and oppressive weather, and the cases are greater or smaller in number, and more or less grave, and the fatality much or little, as the temperature is very exalted or otherwise and continues for long or short periods. July and August are the months which have the more exalted temperature, and when such temperature is most protracted; they are the periods when the bills of mortality of our cities are much increased in their

* Pract. Med., G. B. Wood, 1847, vol. i.

aggregate numerical amounts by the records of the deaths produced by cholera infantum.

3. Hepatic derangements are emphatically the diseases of the tropics and warmer latitudes of the temperate climates. Nearly all the diseases of the tropics, and a large number of those of the hotter latitudes of the temperate zones, involving the alimentary canal, are complicated with hepatic affections. The diarrhœas are not simple diarhœas consisting of merely an irritation of and a hyper-secretion or effusion from the mucous membrane of the intestines, but are bilious diarrhœas. So of the dysenteries, they are not simply such, but they take upon themselves a bilious type. The converse of all this is true of the enteric diseases of the cold climates. Exalted temperature long prevailing in the tropics is the predisposing and often exciting cause of the peculiar or bilious enteric diseases of those regions. The predisposing and exciting cause being absent in the cold latitudes, the complications of liver disturbances are also absent.

Permit me here to digress and by the way of an episode to remark that in bilious dysentery and in bilious diarrhœa the liver, with but rare exceptions, becomes first involved, the bowels subsequently as a result of the abnormal condition of that organ.

It will not perhaps be denied that congestion of the portal vessels, the pouring into the intestinal tube of acrid bile, may give rise to all the phenomena which present in bilious dysentery and in bilious diarrhœa. Certain it is that it is the experience of many, that all remedies which may be addressed to the bowels for their relief, if we overlook the great importance of relieving the liver and of correcting its secretion, will at best prove but little better than palliatives, whilst if regarding the necessity referred to, such remedies as address themselves specially to the liver, relieving it of its engorgement and normalizing its secretion, be employed, such diseases will speedily yield to treatment.

4. The morbid conditions of inflammation, ulceration and softening of the mucous tissue of the intestinal canal, the enlargement of the glands, solitary and acuminate, are not peculiar to cholera infantum, but are present in fatal cases of enteritis, dysentery, etc. Yet these diseases do not exhibit the ante-mortem phenomena which give to cholera infantum its special, if not peculiar, aspects.

5. Infancy is the period of life when mankind are most prone to attacks of inflammatory and congestive diseases, favored doubtlessly by the greater degree of irritability existing at this period of life in all the organs and tissues. As we have advanced, we have seen that hepatic derangements and particularly bilious complications of enteric affections are the diseases of the tropics, or, in other words, of protracted exalted atmospheric temperature. We have seen that cholera infantum is a disease coincident with the hotter months of summer in this latitude. If the protracted exalted heat of the tropics is the great predisposing and exciting cause of the hepatic diseases and hepatic complications there, may it not be reasonably inferred, that the protracted exalted temperature of the canicular days here is to the more irritable or susceptible liver of infants, what the heat of the tropics is to the liver of those who are within the range of its influence? Removal from the tropics to cold or cool latitudes by those afflicted with entero-hepatic diseases of a benign type is followed by restoration of the liver and bowels to their normal condition. Removal of cholera infantum patients from the heat-exalted atmosphere of our cities to the cool atmosphere of the country, and particularly to the ocean-side, is followed by a speedy restoration to health. The tropical invalid when in the cold latitudes is no longer subjected to the predisposing and exciting cause of his entero-hepatic disorder, and, without the use of medicines culled from the diversified departments of the materia medica, casts off the habiliments of disease, and robes himself in the attractive costume of health. A return of the health-restored tropical invalid to his hot clime is too often followed by a return of his disease. A return to the city during the dog-days of the convalesced cholera infantum patient is followed by a return of the disorder, ending too frequently in death. How wonderful the analogy of these diseases under similar circumstances, springing up under like conditions, relieved, nay, cured, by the same hygienic management! Having causes and effects so remarkably alike in some of these prominent phenomena and habits, to what other safe conclusion can we arrive than that there exists an identity between the organs primarily affected and the cause affecting them? But the pathologist who teaches that cholera infantum is primarily and continuously a disease of the alimentary canal, asks, have we not revealed post-mortem traces of inflammation, ulceration and softening of the mucous coat of the intestines, and inflammation and softening of the mucous tissue of the stomach? To all of which we answer in the affirmative. In turn, we ask, does not a great organ, which in the discharge of its functions is as important as any of those previously named, exhibit undeniable evidence of morbid action? Does not congestion and sometimes enlargement of the liver exist? Does not the gall-bladder contain an acrid or otherwise vitiated secretion, and is it not sometimes entirely free from its peculiar fluid? Has not the total absence of normal bile been the universal post-mortem revelation in cholera infantum? Have all these conditions no legitimate interpretation? Must they be viewed as merely accidental, or incidental, as it may suit the taste, fancy, or prejudice of the pathologist? We are ready to declare that when taken into consideration with all the circumstances, and all the phenomena of the disease in the living and the dead, they stand out in bold relief upon the foreground of the picture, as its most attractive and most interesting features.

If the presence, post-mortem, of lesions in the abstract, without consideration of the phenomena of the disease, which present during the lifetime of the patient, will warrant a conclusion in regard to its nature; why separate, in the disease under consideration, the morbid conditions outside of the lesions of the stomach and bowels, from them, and

consider the disease an enteric, gastric, or gastro-enteric malady? Why not group all the lesions, make a unit of them, and consider the disease a hepatic gastro-enteric disorder, primarily and continuously? If we refuse to do this, by what authority, by what common-sense rule, shall we, after discovering lesions of the liver, stomach, and intestines, close our eyes to the liver lesions, and determine the nature of the disease by the other morbid anatomical conditions? Would it not be wiser to group all the lesions, forming an entirety of them, and thus regard the disease as a hepatic gastro-enteric disorder? In doing this, while we will not determine where the disease commenced, in which tissue or organ it first manifested itself, we shall at least not deduce a conclusion prejudicial to any of the organs involved, and therefore our conclusions will be less liable to beget an injurious or negative treatment. The opinion in a general way is regarded as being correct, that it is largely an impossibility, in most diseases, to determine their nature by a consideration of the morbid conditions which are revealed after death, independent of, and out of association with, the morbid phenomena exhibited during life. Lesions revealed post-mortem and the morbid phenomena presented during life must be studied together, each given its true weight and value to enable the pathologist to solve the problem and to accurately determine the true pathology of most disorders. Hence, to determine the nature of cholera infantum, what organ or organs become primarily diseased, whether the liver, the gastric, the enteric mucous membrane, or all combined, we must examine and hold under due consideration not only the post-mortem exhibitions, but the morbid phenomena presented during life. Let us therefore inquire what are the chief of the living phenomena. Vomiting and purging; the stools sometimes, though seldom, evidence the complete absence of bile, but generally the presence of a vitiated hepatic secretion. As the disease progresses, aqueous or serous matter makes up the major part of the evacuations. In violent attacks there is great frequency of stools, accompanied with rapid emaciation, great exhaustion, and even collapse, giving the disease, under such circumstance, a marked analogy to cholera.

Having called before us some of the living revelations of the disease, we will unite them with the lesions discovered after death, take them as our guides in the exploration which we are about to make, and see to what rational conclusions they will lead us. We have already learned that after death congestion of the liver exists, that the gall-bladder is either empty or contains an acrid secretion. At the very dawn of the disease the discharges from the bowels are free from bile, or if there is any present it is abnormal. So far the morbid phenomena during life and the revelations after death are in accord. Frequently, independent of the presence or absence of abnormal bile, we have large and frequent discharges of serum from the bowels and the stomach, and mucous matter is often present in the stools. As the serum and mucous matter are the results or products of a diseased enteric and gastric mucous membrane, the diseased condition of that membrane necessarily preceded the phenomena of which they were the ultimate products. How shall we account for the origin of the diseased condition of the mucous tissue of the stomach and bowels? Thus: having seen as a first revelation during life that the hepatic action was abnormal, we have gained an important stand-point from which to make further observations. Regarding the liver as having its portal vessels congested and this the cause of its functional derangement as exhibited in either the non-secretion or the secretion of abnormal bile, we will endeavor to see what would be the effects of such condition of the liver and a vitiated bile, or the entire suppression of the secretion of that fluid, upon the stomach and bowels. When congestion of the portal vessels which ramify through the liver takes place, there must necessarily follow a damming up behind them of the blood in the channels through which it flows, and, as a sequence, a throwing back of the current upon its source; the capillary rete of the mucous membrane of the stomach and bowels being the source from whence the liver receives its supply of portal blood, when the portal vessels of the liver are congested, as a result of such congestion a congestion of the gastric and enteric mucous membrane takes place by necessity, giving origin to irritation of the mucous follicles, effusion, or hyper-secretion,—the effusion being nothing more or less than an exosmotic action of the venous capillaries of the enteric and gastric mucous membrane. Thus we account for the large and exhausting serous discharges and part of the morbid condition of the mucous membrane of the stomach and bowels, as secondary results of the congestion of the liver.

Simultaneously with the congestion of the mucous membrane of the bowels and the exosmotic action of its venous capillaries, we have acrid bile thrown into the intestinal tube, producing in the highly irritable and very delicate mucous tissue of a babe's bowels follicular enlargement, ulceration and thickening, to say nothing of inflammation, all secondary conditions, resulting from the congestion of the liver. Then we have vomiting, persistent and exhausting. May not this be, is it not indeed, the result of irritation and congestion of the gastric mucous membrane, produced by the acrid secretions of its glands and follicles, arising from the congestion of the gastric capillary rete, produced, as already stated, by congestion of the liver? Add to all this the great probability of morbid influences being transmitted from the bowels, and then say if the account will or will not embrace all that may be necessary to produce the gastric morbid phenomena presented during life, as well as all the lesions which are present after death, in the stomach. Having traced, step by step, the predisposing and exciting cause, the phenomena, the parts involved, and the lesions which make up the totality of this interesting disease, in the regularity, to a certain extent, of their succession, we trust there is good and sufficient cause to consider, certainly for not rejecting hastily, the conclusion that in cholera infantum the liver is primarily diseased, and that

the gastric and enteric disturbances are secondary to, and hence the offspring of, the hepatic congestion, thus indicating that the nature or pathology of the disease consists primarily in congestion of the liver, followed by, and continuous with, and depending upon it, derangement of the bowels and stomach.

The definite fixing of the original or primary seat or starting-point of this disease is all-important, as regards the character of the treatment it should receive from its incipiency to its close. It is of great, nay, almost of vital, importance to the little cholera infantum patient, whether the physician shall regard the enteric and gastric mucous coats, the primary and (may we not add?) the only seat of disease, and the nature of the malady, an enteric and gastric irritation only, or that the disturbance of these tissues, although very active, yet secondary to and dependent upon the congestion of the liver. The physician in his treatment will necessarily be influenced by the pathological view which he may adopt in regard to the disease; hence the importance of the adoption of a correct one.

Before closing, we cannot forbear venturing the opinion that it fully accords with the observation of every medical gentleman, who has paid much attention to this disease, that it cannot be considered as presenting a truly favorable and yielding aspect until the stools evidence the presence of normal bile. If the declaration which we have just ventured be true, as we believe it to be, it points with much force to, and is highly suggestive of, the liver being the important and controlling seat or centre of the malady. Do what we may, treat the disease as we please, the fact still remains, if we fail to produce a flow of normal bile our patient remains ill, with perhaps the malady moderated, not cured: the gastric irritability may have subsided, the evacuations from the bowels be less frequent, less copious, less aqueous, but unless we have normalized the hepatic secretion, our patient will remain in a doubtful condition. Normalize the bile: this done, every bad symptom begins to pass away, and convalescence dawns. Such has been our experience, and, comparing notes with our brethren, we have no cause to be dissatisfied with that experience.

ANIMAL LIGATURES.

BY JOS. BERENS, M.D.

IN his very able article on Ligatures, published recently in the *Philadelphia Medical Times*, Dr. J. R. Haynes concludes that there is no better ligature than silk. The following experiments would seem to show a different result. In the experiments of Dr. Haynes there are not a few grounds for fallacy, or, at least, obstacles to correct deductions. Thus, ligatures of different materials were often applied to the same vessel at the same time, and so close together as materially to interfere with any accurate observation of the separate results. Vessels were invariably chosen so deeply seated as to insure a great disturbance of tissue in reaching them, which, together with the distance of the ligature from the surface, must necessarily have to a great degree impaired the development of symptoms peculiar to the material of the ligature. In many of the experiments, cats were used,—animals which the writer, after over a dozen trials, gave up as altogether unfit for the sort of work in hand, on account of the peculiar nature of their skin, and the strong tendency always existing in them to very great suppuration. Finally, sufficient care was not taken in conducting the experiments to oppose the results derived from the employment of different materials.

In the following experiments these sources of error have been, as far as possible, avoided.

The main points to be decided seem to be, whether any material can be employed for ligatures which will not be more irritating than silk, yet hold the vessel as well, and, in addition, be ultimately absorbed; the object being to obtain in the ligature a body that will be as little as possible foreign to the wound, not only in respect to its irritating qualities proper, but also with regard to the amount of it to be left in contact with the raw surfaces; that is, to get a substance causing so little irritation, and being so readily absorbed, that after securing a vessel the ligature may be cut off close to the knot and the wound closed over it.

With reference to the points involved, three methods of procedure were adopted: 1, setons of the different materials were placed side by side, beneath the skin of dogs. By noting from time to time the effect of these setons in the swelling and general inflammatory symptoms they produced, also the effect of the surrounding tissues upon them, and, finally, by laying open the track in which they lay and observing the condition of affairs within the wound, very effectual comparisons could be drawn in regard to their relative irritating properties, as well as their absorbability.

These experiments were then verified by placing knots of the different materials under the skin. After this, ligatures of the different materials were applied to the arteries of the animals, by which means their qualities in this respect were tested under circumstances very similar to the conditions to which they would be subjected in their employment on man.

The materials experimented with were the imported carbolized gut, prepared on the Lister method, uncarbolized gut, the smallest size guitar-string, peritoneum cut into strips and rolled, as recommended by Dr. Agnew, and beef-tendon dried, split into fine shreds, and twisted. Silk was generally used, also, for the sake of a standard of comparison.

The setons which will be now discussed were, in general, about equal in thickness to three strands of ligature-silk, and were about three inches long. These setons were introduced by means of a long needle, with the production of as little irritation as possible; they were placed side by side, and about an inch and a half apart.

CASE.	TIME.	SILK.	CARBOLIZED GUT.	UNCARBOLIZED GUT.
I.	24 hours.	Inflammatory symptoms.	Slight inflammatory symptoms.	No symptoms.
	48 hours.	Marked welt, and plentiful discharge.	Marked welt; a drop of pus could be squeezed out.	A very slight welt, but no pus.
	3d day.	Unchanged.	Symptoms increased.	No change.
	5th day.	Unchanged.	Much improv'd.	No symptoms.
	12th day.	Opened track, and found much inflammation and excavation.	Symptoms sl't. The seton parted in middle upon slight traction; it had been very nearly absorb'd. Opened, and found slight inflammation.	No symptoms. The seton had been absorbed in centre, and one end had fallen out. Opened, and found track partly healed, and inflammation very slight.
II.	24 hours.	Welt, but no discharge.	Slight welt; no discharge.	Slight welt; no discharge.
	48 hours.	Tolerably well-marked welt, and slight discharge.	Marked welt, and discharge.	Unchanged.
	4th day.	No change.	Welt not so marked, sl't discharge.	A little pus could be pressed out.
	5th day.	No change.	Some improvement.	No welt; no pus.
	10th day.	No change. Open'd track, and found well-marked inflammatory symptoms.	Slight welt; no pus; ends of seton separated on sl't trac'n. Opened, there were few signs of inflammation.	No change; seton still pretty firm. Opened, and found seton reduced to fine thread, with little or no inflammation.

CASE.	TIME.	SILK.	CARBOLIZED GUT.	PERITONEUM.
III.	24 hours.		Symptoms not marked.	No symptoms.
	48 hours.		Moderate welt and disc'rge.	Small welt, no pus.
	4th day.		Symptoms increased.	Decided welt, and brisk discharge.
	9th day.	A moderate welt, and sl't discharge persisted throughout. On opening, marked inflammatory symptoms were found.	Symp'ms much improved. Opened, and found seton softened, and much reduced in size, track not much inflamed.	No change. Opened; seton not much reduced, inflammatory symptoms well marked.
IV.	24 hours.	Slight welt.	No symptoms.	Slight welt.
	48 hours.	Slight welt and a little pus.	Slight welt and discharge.	Marked welt and free discharge.
	4th day.	Unchanged.	Slight welt and discharge.	No change.
	7th day.	Unchanged.	Slight welt; no discharge.	Symptoms improved.
	10th day.	No change. Opened, and found the track very red and inflamed.	No change; seton separated upon slight traction. Ope'd; there was scarcely any sign of inflammation.	Slight welt and discharge; seton still firm. The track was much inflamed and excavated.

CASE.	TIME.	SILK.	UNCARBOLIZED GUT.	TENDON.
V.	24 hours.		No symptoms.	No symptoms.
	48 hours.		Slight welt.	No symptoms except slight welt.
	3d day.		Welt a little more mark'd, slight discharge.	Slight welt, no discharge.
	6th day.		Unchanged.	Unchanged.
	12th day.	The inflammation throughout has been but slightly marked.	No symptoms. Intermediate portion of seton absorbed.	No symptoms. Seton reduced in size; parted on slight traction.

CASE.	TIME.	SILK.	UNCARBOLIZED GUT.	TENDON.
VI.	48 hours.	Slight welt, but marked discharge.	Slight welt, no pus.	No symptoms.
	3d day.	Unchanged.	No change.	No change.
	4th day.	Improved.	Welt almost disappeared.	No symptoms.
	10th day.	Marked welt, and brisk discharge. Opened, and found decided inflammation and suppuration.	Scarcely any welt perceptible. The seton reduced to fine thread which separated very readily on slight traction. On laying open the track, but slight inflammatory symptoms could be discovered.	No symptoms. By exerting considerable force the seton separated. It was flattened and much reduced in size. Laying open the track revealed no signs of inflammation whatever.
VII.	48 hours.	Welt well marked; very little pus.	A slight welt.	No symptoms.
	3d day.	No change.	Moderate welt, and discharge.	Small welt, no pus.
	5th day.	No discharge, but marked welt.	No discharge.	No change.
	10th day.	Discharge and welt. Open'd, the track of seton very much inflamed.	Symptoms very slight. Intermediate portion of seton has been absorbed, and one of the ends fallen out. On opening the track, very slight inflammation could be discovered.	No symptoms. Upon attempting to withdraw the seton it refused to come. Laying open the track revealed the seton buried in the tissue, and apparently adherent, creating not the slightest disturbance.
VIII.	24 hours.	A welt.	Slight welt.	Slight welt.
	48 hours.	Pus and welt.	No change.	No change.
	3d day.	No change.	Welt much diminished.	Welt barely perceptible, but a drop of pus could be press'd out.
	6th day.	Pus flowing freely; the dog's back is wet with it.	Scarcely any swelling.	No swelling, no discharge.
	7th day.	Opened, and found the inflammation marked.	Opened; inflammation exceedingly slight. Seton reduced to one-third its size.	Upon attempting to force the director through the track, great difficulty was experienced. The seton was found flattened, and bound down by bands of tissue thrown over and around it. The track was apparently healed completely, with the seton *in situ*. Seton not diminished much in size.
IX.	48 hours.	Marked welt, no discharge.	Slight welt.	Slight welt.
	3d day.	A little pus could be squeezed out.	Moderate welt, and discharge.	Well-marked welt, no pus.
	7th day.	No change.	All symptoms have disappeared.	Welt much reduced.
	13th day.	Slight welt, no pus. Opened, and found but slight inflammation.	No change. Opened, and found slightest possible inflammation at one end, where portion of seton still remained, the other part having dropped off.	Nothing to mark the track but the protruding ends of the seton, which was reduced to a few loose shreds. The inflammation was less, if possible, than that in the case of the gut.

A large number of experiments with setons, similar to those the notes of which are given, were performed. The cases quoted have been selected with the view of presenting the average result. The animals used were dogs, the hair being either shaved or trimmed close over the tracks of the setons. Cats were also used to some extent, but, proving unsatisfactory, owing to the peculiar looseness of the skin, were abandoned, and the records of experiments on them were not taken into consideration.

A very superficial examination of these notes will reveal the fact that silk was at least as irritating as any of the materials used. The introduction of it beneath the skin was invariably followed by a distinct welt, which, except in rare instances, was accompanied by a discharge of pus more or less marked. In some cases this discharge was so severe as to drench the animal's back for days, while other materials which lay within an inch or two of the silk were creating little or no trouble. Next to the silk come the carbolized gut and peritoneum; reference to the notes will show that these were very nearly similar in the amount of irritation they produced, especially during the first few days, the symptoms of the gut usually ameliorating very greatly after the fourth or fifth day. This mending of symptoms in connection with the gut after some days was probably due to the fact that by that time whatever carbolic acid had been introduced became absorbed or was washed out by the discharge. The peculiar tendency of the peritoneum to produce inflammation is doubtless dependent upon something in the material itself, as every precaution was taken in procuring the membrane fresh from recently-killed cattle, and in preparing it for use. Unlike the carbolized gut, the peritoneum created disturbance as long as it remained in the wound. Next in order comes the plain gut. In the majority of instances the results were most favorable, very little inflammation being induced as a general thing by its presence, and when there was any it was always marked by a welt much smaller and a discharge much less profuse than usually existed in the case of any of the preceding materials. Occasionally there was nothing to indicate the presence of the seton but the two protruding ends,—not the slightest welt or a drop of pus. If the behavior of the plain gut was satisfactory, that of the tendon was eminently so. If the introduction of the plain gut was occasionally followed by no symptoms, that of the tendon was very rarely followed by any signs of inflammation. Indeed, in many cases the protruding ends appeared like large white hairs growing from the dogs' backs, so little disturbance did they create and so closely did the orifices of the track adhere to the seton. In Case VIII., the seton seemed actually to have contracted adhesions and to have become a part of the tissues in which it was buried. The same thing occurred in another case, not quoted.

As to the comparative absorbability, the relations are somewhat altered. In this respect reference to the notes will show that the plain gut stands at the head, then comes the carbolized gut, and after these the peritoneum and tendon. The average length of time—computed from the experiments made, and six special ones—required for the absorption of a seton of the ordinary size, is, approximately, for the plain gut eleven days, for the tendon sixteen days. Of the experiments where knots of the different materials were placed beneath the skin, a few cases will now be given. These knots were all deposited without making any incision. The knot was made and attached to a loop of silk so arranged as to be easily withdrawn; a broad needle was then employed, by means of which the silk was passed under the skin, and the knot was then pulled into the desired position and the silk removed. By thus avoiding an incision directly over the position of the knots, a more correct estimate could be made of symptoms, the swelling, pus, etc., produced.

Case I.—Two triple knots, one of plain the other of carbolized gut, were introduced beneath the skin of a dog. In four days there was a swelling the size of a walnut over the position of the carbolized gut. When this was opened on the fifth day, the knot was discharged softened but not much reduced. The amount of pus was large. The plain gut produced a little swelling, which subsided, however, in about a week; on the seventieth day no trace of it could be found after careful search.

Case II.—Two knots as before. For the first two days there was considerable swelling on the part of both; the swelling over the plain gut had disappeared upon the fourth day, that over the carbolized gut at the end of a week. Both lay after this, creating no disturbance, for two weeks; when searched for at the end of this time they were found very much reduced in size.

Case III.—Triple knots of plain and carbolized gut, peritoneum, tendon, and silk, were placed an inch apart in a dog's back. At the end of the third day the silk, peritoneum, and carbolized gut were exciting considerable inflammation, the other two lying quiet; upon the fourth day the silk was discharged by a touch of the knife from a small abscess it had formed; the peritoneum and carbolized gut were found buried beneath the skin, with evident signs of pus; the plain gut and tendon were found lying inert, except that they were surrounded by a small quantity of lymph.

Case IV.—Two triple knots, one of plain silk, the other of silk saturated in a carbolic solution taken from a bottle of the imported gut ligatures. At the end of twenty-four hours the silk had created but slight disturbance, while the site of the carbolized silk was marked by great swelling and a high degree of inflammation, which culminated in a well-marked abscess on the fourth day, at which time there was only well-marked swelling over the plain silk. On cutting down upon the knots, the plain silk was found buried in a moderate amount of pus which equalled in amount about one-quarter of that in which floated the carbolized knot.

It will be seen that these results verify very strikingly those obtained from the setons,—the silk still presenting its irritating features, the carbolized gut and peritoneum standing next in this respect, the plain gut and tendon manifesting the fewest objectionable points. In addition, Case IV. demonstrates very clearly the irritation which carbolic acid will originate in the tissues.

In the next series the experiments were directed to the ligation of arteries. The ligatures employed were generally about the size of doubled silk; when silk was used it was doubled. The ends of the silk

were cut off close to the knot. The animals used were dogs.

Case I.—The femorals of a large dog were secured with carbolized gut ligatures. Two days after, there was considerable discharge, but only ordinary swelling, both of which subsided in about a week. The right side healed on the eleventh day, the left on the twelfth day. Search being made for the ligatures two weeks after, nothing could be found of them except a few shreds surrounding one of the arteries.

Case II.—Right femoral with carbolized gut, the left with silk. Both exhibited signs of inflammation in a marked degree; this subsided, however, in a week, and the wounds were both healed by the thirteenth day. At the end of the third week the silk was found to have severed the artery, and lay in a cyst that looked like a lymphatic gland. The gut could not be found; the artery was obliterated, though the external coat was not severed.

Case III.—Right femoral with carbolized gut, the left with silk. The wound on right side healed in ten days, that upon the left side requiring eleven days. On the fifteenth day the silk was found close to the surface and buried in pus. The vessel upon the right side was found severed, with nothing left of the ligature but the rounded knot. This was, apparently, causing very little inflammation.

Case IV.—The right femoral of a large dog with *uncarbolized* gut, the left with silk. The wound over the gut healed in seven days, that over the silk requiring eleven days. In three weeks the wounds were reopened. No trace of the gut could be found; the artery was obliterated, but the external coat intact. The silk was found imbedded in pus.

Case V.—The same experiment repeated. The results were very similar, excepting that at the end of two weeks the gut was found almost unchanged in size, though much softened.

Case VI.—In this case the wound over the gut healed in eight days, that over the silk in nine days. In neither case was there much swelling. At the end of four weeks the silk was found lying harmlessly upon the artery, which was severed. No trace of the gut; the artery had been severed.

Case VII.—A large dog. The right femoral with *carbolized* gut, the left with *uncarbolized* gut. The right side showed much swelling and discharge, and did not heal for twelve days. On the left side the swelling was by no means so great, nor the discharge so profuse. It healed in nine days. Upon opening at the end of two weeks, nothing could be found of either ligature but the knots, rounded and smooth, and apparently producing little inflammation, if any.

Case VIII.—Right femoral with tendon, left with silk. The right side healed in eight days, the left requiring twelve. At the end of two weeks the wounds were opened. The tendon ligature was found reduced to a pulp, lying between the cut ends of the artery, with no bad symptoms. The silk was found in a small abscess.

Case IX.—The two femorals of a medium-sized dog secured with tendon ligatures. Perfect healing took place in a week. On opening the wounds three weeks after, the arteries were found obliterated, with external coats intact. Nothing left of the ligatures but two shining knots the size of a pin's head.

Case X.—Secured the right femoral of medium-sized dog with silk, left femoral with tendon. Six days afterwards, the wound upon the left side was completely healed, and presented but moderate swelling, while that upon the right side was still open, much swollen, and discharging freely. On searching for the ligatures, the silk was not to be found, having probably been discharged, the vessel having been completely cut through; the tendon was found much softened, but still surrounding the artery, which, though completely occluded, was not divided.

Case XI.—Ligated right femoral with silk, the left with tendon. For four days the left side wore an unhealthy aspect, while the right side looked well. At the end of a week there was no difference to be observed between the two sides. Both wounds were completely healed on the ninth day. On the tenth day a small lump was felt over the site of the silk ligature; this upon cutting down was found to be a large cyst, which when opened proved to contain the ligature, together with a quantity of pus. Search was made for the tendon ligature upon the thirteenth day; nothing was found of it; the vessel was reduced to a fibrous cord.

Case XII.—The two femorals of a medium-sized dog secured with tendon ligatures equal in size to *fourteen strands* of silk. Both wounds were healed perfectly upon the *tenth day*. The vessels at the end of two weeks were found obliterated; external coats not cut; on the arteries lay the knots of the ligatures, looking like pearls, so smooth and shining were they. They were reduced half in size.

Case XIII.—Right femoral of a large dog tied with a single strand of silk, the left femoral with a fine thread of tendon somewhat smaller than the silk. The wound over the tendon was healed perfectly on the sixth day, that over the silk upon the eighth day. At the end of twelve days the silk was found lying loosely between the severed ends of the artery, causing no trouble. No trace could be found of the tendon ligature upon the left side. The artery was obliterated, though the external coat was still intact. The arteries in this case were quite as large as the femorals in man.

The next material employed was peritoneum, with, however, unsatisfactory results. Three femorals were secured by ligatures of peritoneum, and none of the incisions did remarkably well: one was healed completely on the ninth day, the other two remained open, discharging freely, one for twelve days, the other for two weeks.

Great care has been taken in selecting the cases reported, especially with reference to the position of the vessel and the manner in which it was tied. Carotids and brachials have been excluded, owing to the great disturbance of tissues necessary in securing them; all femorals, also, have been excluded, when there was any bungling in their ligation. The cases quoted may be regarded, therefore, as presenting a fair record of the results obtained, inasmuch as the vessels are all of the same kind, and the operations for securing them very nearly equal in point of disorganization of tissue occasioned by them. Further, the ligatures being deposited so near the surface, their effects could be very readily studied. A review of the notes will show results which might have been anticipated from those obtained from the setons. The silk and peritoneum generally took the lead in the inflammatory symptoms engendered, the attendant swelling and discharge being usually great, and the time required for the healing process somewhat prolonged, when compared with that of the other materials. The carbolized gut comes next in order, but there was no marked difference between it and the preceding ones, there being usually about the same amount of trouble after a ligature of it was deposited; the time of healing, however, was a trifle less,—not quite a day, all cases in which it was used

being considered. After these follow the plain gut and tendon, between which and the foregoing there was a well-defined line of demarcation. The behaviour of the plain gut was very satisfactory, proving it to be readily capable of absorption and but slightly provocative of inflammation, in which respect, however, it was scarcely equal to the tendon, owing perhaps to the processes to which it is subjected in manufacture.* The tendon proved in a marked degree non-irritating; as, for example, in Case XII., where a large bulky ligature, double-knotted round a vessel about one-fifth its size, produced little or no trouble comparatively, and permitted perfect healing of the wound in ten days. The tendon, on the other hand, is by no means so absorbable as the gut.

Approximately, the time required for the complete healing of the wound computed from all the experiments was, for the silk and peritoneum, a little less than eleven days; for the carbolized gut, a little more than ten days; for the plain gut and tendon, between eight and nine days.

The three sets of experiments, taken together, show a result undeniably favorable to the plain gut and tendon.

Comparing the advantages of these two, we find the gut very non-irritating and rapidly absorbable, but making a ligature that scarcely grasps the vessel as firmly as silk, and with rather a bulky knot; on the other hand, the tendon is non-irritating to the last degree, not rapidly absorbable, capable if need be of being made up into threads of almost any degree of fineness, which grasp the vessel quite as well as silk, and at the same time make an excellent small knot. The extreme non-irritating properties of the tendon, coupled with its capacity for fine division, would render its slow absorption of decided advantage, since a vessel could be secured with a strand of it so fine as to produce no irritation, and yet there would sufficient time elapse before its disappearance to render any danger from secondary hemorrhage as remote as in the employment of silk, by which the vessel is usually severed in the course of a very few days. As to the ability of a fine strand to control even a large vessel, attention is called to Case XIII. Here a vessel as large as the femoral artery in man was secured with a ligature so fine as to pass readily through an ordinary cambric-needle. The result, as may be seen, was not in the least prejudicial. Thus it will be seen that the tendon ligature fulfils all the requirements set down at the beginning of this paper for a substitute for the material now in use. It is certainly not more irritating than silk, holds the vessel quite as well, and, in addition, is beyond all doubt ultimately absorbable, so that a ligature of it may be cut off close to the knot and left in the wound. These qualities would render it of great value in amputations. A fine ligature of this kind, applied to the many small vessels that spring after the operation, would certainly have some advantage over the large strands of silk applied now to every vessel that bleeds, however small. At the worst, the ligatures would give no more trouble than the shreds of connective tissue and the like, more or less of which are always covered in by the flaps. In ligations in continuity, also, the tendon ligature would be of value, having all the advantages of silk and lacking many of its objectionable features.

* In making "catgut," the intestines of sheep are used. They are soaked for a considerable time in water, freed from fatty matter, etc., then treated with caustic potash, drawn through perforated iron plates, and subjected to the fumes of sulphur.

TRANSLATIONS.

BUCCAL PSORIASIS.—Dr. Debove contributes to the *Archives Générales*, April, 1874, p. 433, a memoir on this affection, which is not by any means an uncommon one in Paris, though infrequent in this country. Dr. D. does not consider the disease identical with psoriasis as usually found upon the skin, though he retains the name, as more descriptive than that of ichthyosis given by English authors. The appearance of the disease is as follows. It presents itself at first in the form of isolated patches so small as to pass unnoticed. These gradually enlarge, however, and assume an opaline transparent appearance. The epidermic layer of the tongue grows thinner and allows the mucous layer to become apparent; gradually the patch becomes opaque. If these changed portions of the surface of the tongue are now examined with a lens, the papillæ are seen to be enlarged and frequently massed together with epithelial collections.

The usual form of the eruption is that of a V-shaped median patch, situated at the junction of the middle and anterior third of the dorsum linguæ: it may spread, however, over the whole upper surface, and occasionally to a less degree over the inferior parts. When the disease is well developed, the surface of the tongue feels hard and resistant, and occasionally this induration is so marked over certain circumscribed portions as to embarrass the diagnosis materially. A frequent concomitant or sequel to psoriasis in this locality is cancroid.

As to the diagnosis, it is, M. Debove thinks, not difficult. Should the tongue have been irritated by applications, it may be mistaken for eczema. In this case, careful observation, without treatment, will show the characteristic patches taking once more the place of the artificially-created ulcerations. From indurated chancre the eruption is to be distinguished by the much less marked character of the induration, and by its not being accompanied by those glandular enlargements usually found in connection with lingual chancre. Mucous patches are decidedly different in their appearance from buccal psoriasis. Their surface is covered with the tenacious products of secretion, which, while they have a similar opaline appearance, yet when the secretion is scraped off look quite unlike the epithelial covering of the psoriatic patches.

As regards treatment, M. Debove directs total disuse of tobacco in any form, which he regards as having a certain influence towards the development of cancroid upon the psoriasis. Besides this, he advises alkaline washes and the spray of alkaline mineral waters, as well as their internal use. A. V. H.

ANÆSTHESIA BY ELASTIC COMPRESSION.—M. Laborde reports in the *Progrès Médical*, May 30, the results of some experiments made by himself in the production of anæsthesia by Esmarch's bandage. The experiments were made by applying the bandage in the usual way on a dog's leg, when it was found that anæsthesia sufficient to permit of slight operations lasted for some two to three days.

PHILADELPHIA
MEDICAL TIMES.
A WEEKLY JOURNAL OF
MEDICAL AND SURGICAL SCIENCE.

The Philadelphia Medical Times is an independent journal, devoted to no ends or interests whatever but those common to all who cultivate the science of medicine. Its columns are open to all those who wish to express their views on any subject coming within its legitimate sphere.

We invite contributions, reports of cases, notes and queries, medical news, and whatever may tend to increase the value of our pages.

All communications must bear the name of the sender (whether the name is to be published or not), and should be addressed to Editor Philadelphia Medical Times, care of the Publishers.

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SATURDAY, AUGUST 8, 1874.

EDITORIAL.

THE AMERICAN MEDICAL ASSOCIATION.

SOME time since, we had occasion to call the attention of our readers to the curious agreement in sentiment and language between an editorial in the *Chicago Medical Examiner* and one which had appeared the week before in this journal. It will be remembered that no charge of plagiarism was made, although the literal identity of whole paragraphs was complete; and, as our Western confrère took no notice whatever of our second editorial, we concluded that he was not in the habit of reading the *Times*. We are happy to be able to inform our readers that he does now read the *Times*, albeit he writes in no friendly mood and seems sore about something, for in his editorial of July 15 is a long extract from one of our editorials,—this time in quotation-marks.

What if our Chicago brother does contrast his own "liberal and earnest mind, accustomed to work manfully, striving to make the world better," with the "Pecksniff" who wields the editorial pen of the *Times?* What if he does abuse the workman, since he praises the work? Soberly, he writes, "The *Times* is a most valuable journal." Strange, with Pecksniff at its head! We would not, however, have noticed the undignified and feeble personalities which the editor of the *Examiner* has indulged in had we not wanted to say a word about our last editorial upon the American Medical Association,—the alleged cause of his ire. We fear that we have been understood in some quarters as speaking contemptuously of Western physicians, and, in the language of a correspondent, of "stirring up sectional feelings." Nothing, however, was further from our intention. We said, and still maintain, that at the late meeting of the Association only a portion of the profession was represented, and in stating the importance of that portion, it may be, we put our case too strongly. What we wanted to emphasize was—first, the great necessity of the Association being universal in its character; secondly, the fact that at the late meeting the whole profession was not represented. The West is an immense territory, many portions of it even now thickly settled, and all of it destined to be so. Human nature is of very like quality in all parts of our country, and we firmly believe that there are just as good men in the North as in the South, in the East as in the West, in the South as in the North, in the West as in the East. This fact, however, when taken in conjunction with the enormous territory over which the profession in this country is spread, makes it of the more importance that the meetings of the Association should be held in rotation, and that all other measures conducive to making the Association a common point of fusion, a general bond of union between the most widely separated districts, should be taken. In our opinion, this latter function is paramount in importance, and if the American Medical Association fails in this it fails in everything. If space were plentiful, in order to show the justness of our assertion in regard to the late meeting we could cite the names of twenty prominent Eastern medical men who were not there to where our Western brother cites one that was there; but our readers can do this for themselves.

CORRESPONDENCE.

TO THE EDITOR OF THE PHILADELPHIA MEDICAL TIMES:

SIR,—My attention has just been drawn to a review of my book on "Hay Fever," which appeared in your issue of the 15th of November, 1873. With your permission, I wish to be allowed to correct one or two statements made by your reviewer, and to make a few observations on the subject of your American autumnal catarrh.

Your reviewer, in speaking of my experiments, says I found "the intensity of the catarrhal symptoms to correspond closely with the amount of pollen found floating in the atmosphere, as demonstrated by the microscope, and that both reached their highest development in the vicinity of grass-fields during the time of hay-making." The former part of this statement is quite correct, but the latter does not express exactly

what I intended to say in my remarks on this part of the subject. As a matter of fact, hay fever does not depend upon hay-making. The disease generally reaches its highest point of intensity before hay-making begins in England, and declines rapidly as soon as this becomes general. The malady would show itself just the same if no grass were to be cut and made into hay, as it does now each season with this process going on. The development of the specific symptoms entirely depends upon the formation and distribution of pollen during the flowering period; and if this latter process is allowed to complete itself before the grass is cut, no hay fever will be produced by its being made into hay.

Your reviewer also represents me as holding the opinion that hay-fever patients often belong to a gouty family. I may, in an indirect manner, have conveyed this impression in my book, but I am not aware of having done so, and so far as I have been able to inquire into this matter I have not found that those who suffer from hay fever are more liable to gout than those who do not.

At the time my volume went to press I had not seen or heard of Dr. Wyman's book on "Autumnal Catarrh." I have, however, since then read it carefully, and have been much interested with the cases he has collected, and with the numerous facts he gives in connection with these; but, contrary to what your reviewer says would be the case, my reading it has not altered my views on the cause of hay fever, but has helped very much to strengthen the opinions I previously held. So far as I could gather from his book, Dr. Wyman had, when he wrote, no idea of the real nature of the cause of either hay fever or autumnal catarrh, and from a correspondence I had with him since he read my book (whilst over in England) I judge that he believes the cause of the latter disease to be "still a mystery." Our "June cold" or "summer catarrh" (hay fever) is undoubtedly due to the pollen of grass; and after a careful perusal of Dr. Wyman's book I am convinced that American autumnal catarrh and English hay fever are one and the same disease and due to the same cause, —namely, pollen; the one to the pollen of grass, and the other to the pollen of Indian corn, which latter, according to Dr. W.'s book, flowers just at the time your autumnal catarrh is most prevalent.

A few well-conducted experiments tried on the same plan as some of those described in my book would go far to settle this question; and if some of your physicians or laymen who are affected with this disorder will devote themselves to the work for a single season, I have very little doubt that the facts gathered in your own country will be found to support the conclusions I have arrived at by the experiments I have gone through in England. Hoping that some one will be found who is able and willing to devote himself to this inquiry,

I remain, sir,

Yours respectfully,

CHARLES HARRISON BLACKLEY, M.D., M.R.C.S.

STRATFORD ROAD, MANCHESTER, ENGLAND, July 15, 1874.

TO THE EDITOR OF THE PHILADELPHIA MEDICAL TIMES:

I OBSERVED in your issue of July 11, 1874, an article written by Dr. E. P. Bernardy, in which he claimed the honor of being the first American physician to recognize the value of the hydrate of chloral as a therapeutic agent in the treatment of puerperal eclampsia. In a recent number, July 25, I see that Dr. William B. Atkinson disputes the claim of Dr. Bernardy, and assumes that to himself (Dr. B.) belongs the credit. He claims that he used the remedy in question in a case of puerperal convulsions as early as October 26, 1871. I now propose to dispute the claim of my friend Dr. Atkinson, and endeavor to establish priority for Dr. Isaac S. Eshleman. I am thoroughly convinced that Dr. Eshleman was among the first, if indeed he was not the first, of our practitioners to use the new remedy. I know that at a meeting of the Northern Medical Society, in the winter of 1869–70, chloral was the topic of discussion. One after another spoke upon the subject: the great majority not having even granted it a trial, Dr. E. was the only person present who could say anything in its favor. He mentioned at that time that he had used it successfully in cerebro-spinal meningitis and tetanus, and said he believed it applicable to all forms of convulsions.

In order to show how early he had the audacity to use it, I am tempted to mention an incident. He was giving it in a case of convulsions, consequent, probably, upon chronic softening of the brain. In his absence, a brother-physician called to see the case, and horrified the family by saying that chloral was a remedy he had never used, and that all he knew about it was that "scientists had used it in experimenting upon cats and dogs."

On the first day of November, 1870, the doctor was called to see Mrs. S., residing on Wallace Street, who was laboring under puerperal convulsions of the most violent type. He gave her chloral in large doses, and the convulsions were soon under control. I should add that in this case he also resorted to venesection. You will bear me out in the statement, Mr. Editor, that this case occurred almost one year prior to the case mentioned by Dr. Atkinson.

Again, on the 10th of March, 1871, he was summoned at midnight to see Mrs. K., residing on Ridge Avenue. She had been all night in the hands of a homœopathic practitioner. The convulsions were frightful in the extreme. The doctor immediately ordered chloral, and tried to bleed her, but no blood flowed. The chloral was administered quite freely, and as soon as she came under its influence the convulsions ceased, and she made a very satisfactory recovery. She took about one hundred and twenty grains in twelve hours.

This second case was at least eight months prior to Dr. A.'s case. Since this time the doctor has used it with the best possible results in numerous cases of puerperal eclampsia, and has never failed to add his testimony to its efficacy in convulsions, no matter how caused. He has been particularly fortunate with it in the convulsions of quite young children. In cases of

cerebro-spinal meningitis and tetanus it has been almost invaluable.

Yours, etc.,
EMORY ESHLEMAN, M.D.

1333 GIRARD AVENUE.

PROCEEDINGS OF SOCIETIES.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

A CONVERSATIONAL meeting was held April 8, 1874, at 8 o'clock P.M.

The PRESIDENT, DR. W. L. ATLEE, in the chair.

Dr. W. B. ATKINSON stated that he had recently delivered a child at full time whose left arm terminated about one and a half inches below the elbow. It did not present the appearance of having been amputated in utero; but there were two or three processes at the extremity, evidently rudimentary fingers. No cause could be assigned, save that the mother at the third month was frightened by lightning and grasped her left arm convulsively just below the elbow. Last spring, in one of his cases delivered during his absence, the child was hideously deformed, having but one nostril, the palate cleft from the nose completely through, and the eyes placed widely apart. In this instance the father was somewhat similarly deformed. This was her second child, the first one being also deformed, but not to so great an extent. He alluded to a case recently published, where the mother had marked her child with a toad on the back.

He could not offer any theory as to the cause of these deformities. If the sight of a deformity, etc., could produce it, then every woman in a certain part of this city ought to give birth to monsters, as there has lived for many years a man with the well-known fire-mark covering the whole face. This man is an object of great aversion to the females of that section, yet he (Dr. A.) had, although largely engaged in obstetrics in that section, never yet seen anything approaching it. He also alluded to an infant which, during the illness of an older child, had been neglected, where it was found that a worsted thread from its sock had become wound around the small toe, and had cut completely down to the bone. This was an illustration of the effect of elastic bands, as now proposed abroad for amputations, etc.

Dr. W. L. ATLEE stated that in his early professional life, while practising in the village of Mount Joy, Lancaster County, Pa., he attended a farmer's wife in confinement, who, immediately on the birth of the child, asked if it had a hare-lip. The child *had a hare-lip;* but the question having been evaded, she peremptorily repeated it, and said that she knew it was so deformed. She accounted for it by saying that in the early period of her pregnancy she had attended a public sale at a neighboring farm-house, where a number of people had assembled, and among them was a man with a hare-lip, who was continually obtruding himself upon her. His deformity made a deep impression upon her, and continually haunted her up to the period of parturition.

Soon after, a converse occurrence took place: a young married lady, three or four months pregnant, was much frightened by a fire which threatened to burn down her parents' house. In her distress, she bewailingly covered her face with her hands. The old women around her tore her hands from her face, and said, "You will mark your child." A strong impression was made upon her mind, and, as in the other instance, she could not divest herself of the idea that her child would have a "fire-mark" on its face. Such, however, was not the result.

In reference to the agency of the umbilical cord in producing deformities, and even death, of the fœtus, Dr. Atlee mentioned a singular case of suicide by a fœtus in utero.

A lady was taken in labor within a week of her full period of gestation, but had felt no motion for three weeks before. It was a foot-presentation, with the back of the child to the front of the mother. The child was dead and putrid, and the liquor amnii was of a dirty dark color. On investigating the cause of death, a very tight knot, which totally impeded the circulation, was found in the cord about nine inches from the body of the child.

Dr. F. J. BUCK mentioned a case at the *Free Excursions* last summer, where both legs of a child terminated below the knee-joint. The mother ascribed it to an impression made by the beggar on Chestnut Street, with whom all were familiar.

Dr. STUBBS mentioned the case of a woman who put her hand in a rag-bag during pregnancy, and a mouse ran up her arm. She was greatly startled at the time, and when her child was born there was shown a line resembling mouse-fur running up the arm.

Dr. A. NEBINGER then read a paper on "The Pathology of Cholera Infantum."

Dr. ATLEE remarked on the paper of the lecturer that there was a great difference between good arguments upon false data and those upon correct data. The reasoning of the lecturer was plausible, but in his opinion it was based upon erroneous premises. In most cases of cholera infantum, of choleroid, diarrhœa, and dysentery, he believed that the affection of the liver was secondary, and that if the incipient stages of these diseases were properly treated, very little or no hepatic trouble would be manifested, and consequently no necessity of directing any special remedy to the liver. The portal circulatory system is so constituted that it takes its origin in the chylopoetic viscera and the alimentary canal, conveys the blood thence to the liver, and again ramifies throughout the structure of the latter organ. Consequently, any morbid action developed in the mucous membrane of the alimentary passages may more or less affect the condition of the blood in the vena porta; and as the current is from the intestines to the liver, the latter may suffer from the presence of this impure fluid. The primary irritation being in the *primæ viæ*, the secondary effects are manifested in the liver. Now, the first indication in all these derangements of the bowels is to get rid of the cause of the intestinal irritation; and if this be accomplished at the very earliest period, there will be very little danger of hepatic trouble. The safest and most successful plan, according to Dr. A.'s experience in the treatment of these and other intestinal diseases, is, immediately on their accession, to order, for an adult, a dose of castor-oil (f℥j) and laudanum (gtt. xv to xx), to remove the irritating contents as well as the mucous irritation, and, if need be, afterwards to give sulphate of soda (gr. viij) and opium (gr. ¼) every two hours, with an additional dose every time the bowels are disturbed, and to continue this treatment until natural stools are induced. If this be done, in ninety-nine cases out of a hundred not a particle of medicine need be prescribed for the liver, as its action will be normal. If, however, the irritating ingesta be locked up in the bowels by astringents, cholera-mixtures, etc., the irritation will be aggravated and prolonged until the portal circulation will become poisoned, and the very results so well described by the lecturer will be developed, and his diagnosis confirmed

by post-mortem examination. The therapeutic success of such a plan of treatment illustrates infinitely better the pathology of these diseases than do any autopsical examinations, and at the same time saves the trouble of verifying our diagnosis by the latter means.

Dr. A. remarked that there were more potent causes than heat operating in large cities to originate and maintain cholera infantum. No doubt the high thermometric condition of the atmosphere had more or less influence; but when we go into our markets, and see the character of our milk and other articles of food by which our children are fed, we find a more efficient cause of intestinal disturbance. And when these sick children are sent into the country, it is not so much a diminution of temperature (which does not necessarily follow) as it is a change in the purity and freshness of their food that operates towards their recovery.

Dr. A. said that the poor liver had been charged with committing a great many sins. He thought it had been too much slandered. He had endeavored to explain his views in reference to the abnormal condition of the liver,—that it was merely a secondary affection, resulting from a neglected primary irritation in the intestinal canal. He would ask the lecturer to explain how heat operated to produce the condition of the liver so graphically described by him.

Dr. NEBINGER said that in his opening remarks upon the disease under consideration he took opportunity to observe that the pathology of cholera infantum was an open question. The remarks of those who had followed him in the discussion being adverse to the proposition put forth, that the liver is the starting-point of the disease, must be regarded as an indorsement of his declaration that the pathology of cholera infantum is yet an unsettled question. He said he was not surprised that his views had met with adverse strictures: indeed, it was precisely that which he had expected. He felt that to-night, as was his habit in the Society upon other occasions not very remote, his flint would strike the steel of his fellow-members, and from the blow heat and light would be evolved. He had not been disappointed.

Dr. Atlee in his remarks said that the first indication in cholera infantum is to get rid of the cause of the irritation of the intestinal tube, and if this be done at the very earliest period there will be very little danger of liver-trouble. This declaration of the doctor's is at best a little vague and unsatisfactory, and not reconcilable with the conditions which must have been met with in almost every, yea, he could add, in every instance of cholera infantum. The remarks of the doctor are, virtually, that if the bowels be promptly freed of their contents,—that is, of that which they contained at the time the attack of cholera infantum set up,—the freeing of the bowels would not only prevent the involvement of the liver, but would give health to the child. Certainly the doctor has forgotten that, in the vast majority of instances, when the physician has been summoned to treat cholera infantum, long before his arrival at the couch of the patient, without the action of any medicine, the bowels have been freely emptied, all irritating ingesta removed, and there is pouring out from the bowels but little if anything else than serum and mucus; yet, notwithstanding this, the morbid action of the bowels goes on, the frequency of the evacuations increasing, and the exhaustion of the patient hourly grows worse. If, as Dr. Atlee has said, the mere casting out of the contents of the bowels will prevent the liver-trouble, as in every case of cholera infantum the ingesta are fully removed in a few hours at most after the dawn of the attack,—indeed, in some cases where the result has been fatal, death has occurred in a few hours after the commencement of the attack,—in these the bowels have been entirely freed from ingesta, yet post-mortem examinations have revealed congestion of the liver, and the gall-bladder empty or containing an abnormal bile. According to Dr. Atlee's views and statements, the emptying of the bowels should have prevented the hepatic trouble; yet the facts, not the fancies, are that the bowels were emptied, and promptly, too, but the emptying of them did not prevent hepatic trouble, as it should have done if the doctor's hypothesis is correct.

The doctrine of elimination of the contents of the bowels by evacuants, as being potent in the cure of bowel-affections, and especially of the use of castor-oil in repeated doses being a sovereign remedy for enteric diseases, and especially that Johnson, of London, has reported cases of cholera successfully treated by the use of castor-oil only, we are all, no doubt, quite familiar; but how many of us have confidence in Johnson's castor-oil treatment, and how many of us are ready to rely upon such treatment in cholera, is another matter. "I hesitate not," Dr. Nebinger said, "to speak for myself, and to say that I do not believe that a case of cholera was ever cured by the use of castor-oil or other evacuants, Johnson to the contrary notwithstanding."

In regard to all Dr. Atlee has said about the treatment of cholera infantum by locking up the irritating ingesta in the bowels by the use of astringents, cholera-mixtures, etc., Dr. Nebinger said that practically he did not know anything about such treatment, nor, with his views of the pathology of cholera infantum, could he ever know from practice anything about such treatment of the disease. The pathological views which he held in regard to cholera infantum, and which he had endeavored to present to the Society, suggested to him a rational treatment, which he pursued in the management of the disease, and with the results of which he was satisfied, and caused him, moreover, to regard as irrational such treatment as named by Dr. Atlee, and to conclude that when such treatment has been used with apparent success the *vis medicatrix naturæ* of the little patient has tided it safely over both the attack of the disease and the attack of the doctor.

Dr. Atlee says there are more potent causes than heat operating in large cities to originate and maintain cholera infantum. "Go," he remarks, "into our markets, and see the character of our milk and the other articles of food by which our children are fed. We find in them more efficient causes of intestinal disturbance than high thermometric condition of atmosphere." The doctor must not jump at conclusions: it is not safe to do so. It is true some babes are fed upon improper diet; but impure food will not account for the production of the disease in those—and such there are—who are not fed upon bad milk or an unwholesome diet of any kind: therefore, for these the doctor will have to find another cause for the production of cholera infantum in them than unwholesome diet. Dr. N. said he thought Dr. Atlee might find the cause to be the one he had suggested in his paper,—exalted atmospheric temperature, long continued. Dr. Atlee had asked him to explain how heat, or rather solar heat, produces congestion of the liver in cholera infantum. Dr. N. said he could not tell how solar heat congested the liver, but he presumed it did through the agency of the nerve-centres; but it does not follow because we cannot tell how a thing is done that it therefore is not done at all, as, for example, we know sunstroke is produced by solar heat; but because we cannot tell how it is produced, it does not follow that the sun or solar heat is not the agency by which sunstroke is produced.

Dr. N. said that he was in full sympathy with Dr. Atlee in the matter of the abuse which has been heaped upon the liver, and that he was aware that it had been charged with being the cause of bodily discomforts when it was innocent of all wrong; yet,

because it is occasionally charged with wrong-doing unjustly, we must not therefore conclude that it is never in fault. The spinal cord, the lungs, the heart, the spleen, the stomach, have all suffered from false accusations, and we must not neglect to say that the womb, above all other organs, has come in for an immensity of slander, and has not only been made the scapegoat of the wrong-doing of other organs, but it has, most unfortunately, been the shield of protection for ignorance and rapacity.

Dr. ESHLEMAN said he had no fixed idea of its pathology. He suspected that the primary lesion lay anterior to that of the alimentary canal and the abdominal viscera, *in the nerve-centres*. Remedies addressed to these, such as chloral, bromide of potassium, opium, and cool air, were no less essential in treatment than bismuth, aromatic syrup of galls, good milk, beef-juice, pepsin, brandy, and ice-water. So treated, he had not found the disease fatal.

PATHOLOGICAL SOCIETY OF PHILADELPHIA.

THURSDAY EVENING, JUNE 11, 1874.

THE PRESIDENT, DR. WM. PEPPER, in the chair.

DR. L. A. DUHRING presented a specimen of *angeioma with sarcomatous degeneration*, and made the following remarks:

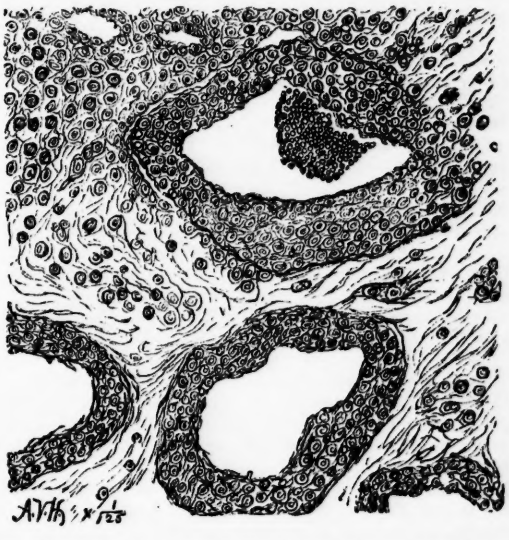
"The tumor which I present for examination possesses the following history: The patient was an Irishman, 50 years of age, with good general health, who stated that the growth began fifteen years ago. It first showed itself in the form of a small elevation or wart-like prominence. Its growth was very slow, especially for the first few years; of late it had evinced a disposition to more rapid development. At its commencement it was not painful, and caused him no inconvenience whatever; but since it had increased in size, within the last few years, it had been somewhat painful when squeezed or accidentally struck, and had been a source of 'worriment' to him.

"Upon inspection there was found a somewhat pendulous tumor, the size of a large hazel-nut, pedunculated, and seated upon the tissues of the left arm, at about the insertion of the deltoid muscle. It was soft and spongy to the touch, but could not be manipulated without causing decided pain. After prolonged gentle pressure it became quite distorted, but immediately regained its former shape and general appearance upon the removal of the force. It was a flaccid tumor, hung loosely from its pedicle over the skin of the arm, and could be moved readily from side to side in all directions. In form it was roundish and somewhat oblong, and possessed a smooth though corrugated surface. In color it was, when in its normal condition, dark-reddish, with a purplish hue in its most dependent parts; when exposed to manipulation or pressure the color varied, and showed many shades of red and purple. The growth manifestly contained blood in large quantities, which could be made to accumulate or disperse, according to the manner in which it was handled. There was no sign of ulceration or breaking down anywhere upon its surface, its structure throughout appearing to be uniformly distributed. The growth bore the general features of an erectile or vascular tumor. There was no pulsation perceptible.

"Immediately after removal, which was accomplished by an elliptical incision about the pedicle, including a considerable portion of skin and subcutaneous tissue, the tumor became softer, although it still retained its general form. But little blood escaped during the operation, and the patient made a favorable recovery. The tumor was cut open directly after excision, and showed a grayish surface with numerous reddish puncta disseminated in an irregular manner. The tissue was soft and spongy, although quite firm, and exhibited no appearance of disintegration. It possessed a reticulated structure.

"Upon pressure, a very small amount of yellowish fluid mixed with blood oozed out, but there was no pure blood to be seen.

"A piece of tissue taken from the centre of the tumor



was hardened in a weak solution of chromic acid and alcohol, and in the form of thin sections submitted to the microscope. With a low power an irregularly-formed tissue is seen, characterized by bands of structure of various size, shape, and distribution, which are present in all parts of the field, constituting a large part of the growth. These bands are made up of connective-tissue elements, and comprise the reticular work already referred to as seen with the naked eye. It is at once observed that much of this mesh-work consists of blood-vessels which have been cut in the section, some evenly and others obliquely, giving rise to a number of appearances, and which go to render the tissue so ununiform. These blood-vessels are present in all sizes, some still containing coagula of blood, while others are empty, their walls alone being seen as rings of connective tissue; some have collapsed, while others have retained their naturally distended form, giving a net-like look to the structure. These openings are larger and more developed in some parts of the field than in others.

"In addition to this mesh-work of vessels, there is another element in the growth, of almost equal importance and interest. In these bands and rings and throughout the whole tissue there exists an active and abundant cell-formation, which may be seen permeating all parts of the tumor, and which has a large share in the growth as seen at the present time. These cells are for the most part round or roundish, contain a large nucleus, and vary greatly as to uniformity of size. They are crowded together in places, and are very numerous everywhere in the field. They are imbedded or seated upon a light, delicate connective tissue, from which they appear to spring. They may be seen invading the walls of the blood-vessels, where they produce an altered and abnormal appearance, destroying their sharpness of outline and changing their usual conformation. In certain fields the cells may be

noticed collected together or arranged in strings, like so many spores of a fungus. The interest which concerns this little tumor rests with the results of the microscopic examination. Clinically, it was judged to be a simple vascular tumor or angeioma, of the cavernous variety; the view of its being of a sarcomatous nature never having been entertained, and yet the microscope reveals this element in a marked degree. Viewed simply as a microscopic specimen, apart from its gross physical or clinical appearances, it might with almost equal justice be ranged either among the sarcomata proper or with the angeiomata.

"In certain examples of sarcoma the vascularity is quite extensively developed, the veins especially being enlarged; but in most cases this condition does not exist, the vascular structure being of secondary importance. In the tumor under consideration the veins, arteries, and capillaries appear, however, to be too largely and conspicuously developed to be viewed in the light of a secondary formation. The walls of the vessels, which we have seen to be infiltrated with cells, also show the vascular elements to be of earlier date than the cells, which seem to be just beginning to invade these structures. From these facts, there can be no question but that in the case before us we have an angeioma of the cavernous type, in which has developed, as a secondary formation, round-celled sarcoma."

Dr. R. M. BERTOLET agreed with Dr. Duhring in his idea as to the relation of the two conditions, the vascularity and the subsequent sarcomatous degeneration. As to the other view, which would make it a sarcoma originally, in which we have this development of huge blood-vessels later, he did not think it so reasonable. It is true that some of the cases of *fungus hæmatodes* are examples of extremely vascular sarcoma, but such tumors are generally much more rapid in their growth, and present other characters different from those of the angeioma. The nævi or mother's-marks upon the skin are very liable to be developed into some form of angeioma, which may remain innocuous for a long period, but if there is a rapid and multiple development of pigmented growths upon the skin, the probabilities are that we have to deal with melanotic sarcomata.

Dr. C. H. BURNETT presented *polypi from the ear*, with portions of a *necrosed incus*, and read the following history: "These polypi were removed from the left ear of a boy 7 years old, at present in the surgical ward of the Presbyterian Hospital, where he has been operated upon for lesion of the knee-joint. The patient is of a remarkably scrofulous diathesis, having at present a scrofulous ulcer on the neck beneath the affected ear. I found a copious sanious and offensive discharge issuing from the left meatus when my attention was called to this case by the surgeon then in charge of the ward. The parents state that the discharge has existed for two years.

"There has been mastoid disease on each side, to the greatest extent, however, on the left side, where I found a wide depression, with a corrugated surface, as large as the end of one's thumb.

"On the right mastoid process, close to the auricle, directly behind the meatus, there is a small, round, and deep depression in the bone, marking, in all probability, the exit of a former sinus.

"Upon inspection of the left meatus, I found a polypus as large as a pea lying half-way down the passage, attached to the antero-superior wall by a short, thick pedicle. I removed the polypus by means of Blake's-Wilde's snare, and touched the cut surface of the pedicle with a drop or two of chloro-acetic acid. Its removal was attended by a slight hemorrhage. After the removal of this polypus and the cessation of the bleeding, I discovered a longer, deep-red body, lying on the floor of the meatus, which at first sight I considered a clot of blood, and, seizing it with a pair of forceps, I gently removed it. As soon as it was removed from the auditory meatus, I discovered that it was an organized body, containing in its structure a large amount of fluid blood, and imbedded in it I found the accompanying piece of bone, which is a portion of one of the ossicles. Dr. H. Allen has examined this bone for me, and has pronounced it the long process of the incus. I could find no necrosed surface in the ear from which this piece of bone could have come, which fact would remove any doubts as to its true nature. The organized vesicle-like body of polypus containing fluid blood was placed in a solution of equal parts of water and glycerin, which soon became deeply stained with blood, leaving the membranous sac quite pale and flaccid.

"The first polypus, placed in the same preservative fluid, has retained its original shape and brownish color.

"The fetid discharge has ceased, in three weeks since the operation, under the use of daily syringing with Labarraque's solution and water (f℥ss—Oj). The amount of hearing in this case could not be obtained, because the child belongs to that class, now and then seen in hospitals, which makes no reply to its superiors, but converses freely with its playmates, apparently hearing all that is said. There can be no doubt that the boy is deaf in the left ear, for inspection since the cessation of the discharge reveals the entire loss of the membrana tympani and all those portions of the ossicula auditus usually seen from the meatus. What the condition of the bodies of the ossicles may be, it is impossible to state. I believe the portion of the incus removed with the second polypoid growth became detached by a process of necrosis, but was mechanically retained in the meatus by the first polypus, which entirely occluded the meatus. The second growth, to which the long process of the incus adhered, was in all probability a polypus in the process of formation."

THE PRESIDENT mentioned a *morbid discharge* which he had met in the morning for the first time. It is the condition known by the French writers as *albuminoid expectoration*, which presents itself sometimes after paracentesis of the chest. He had, in connection with Dr. Thos. J. Yarrow, drawn off from the chest of a patient with hydrothorax, probably connected with tuberculous disease of the pleura, seventy-five ounces of serum. The operation was easily performed with the aspirator, and gave the patient great relief; but towards the end of the operation there was grating of the roughened pleura against the canula, which caused him to desist. There also occurred, subsequently, some troublesome cough, which was, however, relieved by deodorized tincture of opium. The fluid drawn off promptly coagulated, the latter portion becoming semi-solid, like soft jelly.

In about eighteen hours after the operation he began to expectorate very freely a peculiar matter, consisting of an abundant muco-fibrinous coagulum floating in a straw-colored serum, the amount raised in twenty-four hours being at least three gills. It resembled so closely the fluid drawn off from the chest as to attract the notice of the family. This peculiar expectoration corresponds with what has been described, especially by the French physicians. The amount which is reported to have been expectorated in some cases much exceeds what occurred here. Various explanations have been offered for this phenomenon, but no one seems to have entirely established itself. Some have attributed it to laceration of the lung and the escape of some of the pleural effusion through the bronchi, but such did not occur in this instance, since there was no hæmoptysis, pneumothorax, or other evidence of puncture of the lung-tissue.

It has also been suggested that this expectoration depends upon a rapid collateral œdema or transudation

into the bronchi, which occurs while the lung is expanding and returning to its original state; and this appears the most probable view of the matter.

The patient had a dangerous syncopal attack six or seven hours after the operation, from which he was, however, aroused by stimulants.

Dr. J. EWING MEARS asked the President whether the formation of the coagulum in the fluid drawn off would not imply to a certain extent the existence of inflammation. He had observed that wherever this had occurred in fluids derived from tapping the abdomen, there had been inflammatory action. He had never observed it in pure *hydro-peritoneum*, in which the fluid is a true transudation and resembles more or less closely the serum of the blood.

THE PRESIDENT replied that while he thought that the presence of a large amount of this coagulum favored the view of inflammatory action of a certain grade, he had also observed it to take place more or less in all effusions. In effusions attending cirrhosis of the liver, where, although the peritoneum was altered, there had been no inflammation, he had frequently seen after ten or twelve hours flocculi of this substance. He had seen it also in the fluid of non-inflammatory hydrocele and in pleural effusions attending heart-disease. Where, however, as already stated, the amount of this coagulum is large, he considered it to a certain extent evidence of inflammatory action.

DR. MEARS said he had asked the question more particularly in relation to the character as possessed by the expectoration.

DR. BERTOLET said that it was readily conceivable that we might have this expectoration without any previous inflammation, if we considered the analogous process where, in consequence of occlusion of a smaller bronchus, either by the entrance of a foreign body or by plugging with inspissated secretions, there is brought about a collapse of the lung-tissue supplied by the obstructed bronchiole. The wedge-shaped portions of the lung in which there has been thus acquired atelectasis soon become hyperæmic, followed by an exudation of serum and blood. This subsequent œdema or splenization does not imply an inflammatory condition, but results from mechanical causes, such as stasis and the removal of the internal pressure of the atmosphere. In the operation for paracentesis, the external pressure is removed from the more or less completely collapsed lung, and again the conditions are favorable for the transudation of the liquid and solid constituents of the blood into the air-vesicles; but here, since there is no obstruction of the bronchioles, it is readily expectorated.

Dr. B. suggested the use of Donnie's liq. potassa test for pus, in another instance, with a view of determining more precisely the condition of the expectorated matter.

REVIEWS AND BOOK NOTICES.

A MANUAL OF TOXICOLOGY, including the Consideration of the Nature, Properties, Effects, and Means of Detection of Poisons; more especially in their Medico-Legal Relations. By JOHN J. REESE, M.D., Professor of Medical Jurisprudence and Toxicology in the University of Pennsylvania, etc. 8vo, pp. 507. Philadelphia, J. B. Lippincott & Co., 1874.

As the result of the labors of a skilful, earnest, and enthusiastic practitioner and teacher, who has devoted some of his best efforts to the unravelling of medico-legal intricacies, this new and original work creates for itself an elevated position among the standard books of its special department. The particular branch of medical jurisprudence which it investigates—that devoted to the field of toxicology—needs all the light that the most powerful lenses of thought and intellect can concentrate upon it. Human life may hang upon the faintest diversity of hue in a chemical reaction, and justice be made more hopelessly blind by the dust thrown into her eyes by the pretentious and ignorant of both professions. It is fearful to think of the risks of life to which the innocent have been subjected, and of the insufficiency of scientific testimony which has afforded loopholes for the escape of the guilty, as we reflect upon the great criminal trials of the past. Doubtless, a single fallacious test, a color that told a false and fleeting story, a symptom whose very intensity was an unreliable diagnostic sign, were sufficient, in numerous instances, to acquit the guilty or condemn the innocent. In these days, however, each new case of medico-legal interest seems to develop the necessity of thoroughness in the appreciation of every detail of test and symptom, and of a completely exhaustive analysis, which should be followed by testimony in the witness-box the accuracy of which should be unquestioned and unquestionable. As Prof. Reese justly remarks, the great leading idea that ought to govern the toxicologist in all his chemical investigations is, not to see with how small a number of tests he can prove the presence of the poison sought for, but rather how many different tests he can adduce to prove his point. The author states, however, that a veteran chemist has rather boastfully asserted on the witness-stand that, in making an analysis for poison in a capital case, if he had satisfied himself of its presence by one or two tests, any further experiments were quite unnecessary.

A poison is well defined by Prof. Reese as "a substance capable of producing noxious and even fatal effects upon the system, no matter by what avenue it be introduced; and this, as an ordinary result, in a healthy state of the body, and not by a mechanical action." The question of *quantity* is sensibly left out, for "the mere quantity of the substance required to destroy life cannot be made a ground of distinction between a poisonous and a (so-called) non-poisonous article."

The number of poisonous substances detected in the blood, the secretions, and the tissues and organs of the body, has wonderfully increased since the days of Orfila, whose published account limited them to comparative numerical insignificance when contrasted with those detected by more recent investigators. The effects of various physiological conditions are ably discussed in a chapter on the "Mode of Action of Poisons on the Animal Economy," the author stating that "there is good reason to believe that the poison is active only while circulating in the capillary blood-vessels. It is a very common mistake to suppose that death is caused by the very poison discovered in the stomach; the death being, in point of fact, attributable to the absorbed portion only (except in the case of the corrosives). The quantity remaining in the stomach after death is the surplus of what was necessary to kill."

A chapter of especial interest to the defendant in a medico-legal case and his counsel includes an inquiry into the possibility of imbibition of poisons after death. "Is it possible," says the author, "for a dead body to imbibe a poisonous substance from the soil in which it has been interred? Is it possible that a poison introduced into the stomach or the rectum, or by the hypodermic method, after death, should pass through the tissues by imbibition into other viscera of the body, so as to give rise to the suspicion of poisoning, when, in reality, the death had resulted from a different cause?" In regard to the first branch of the proposition,—cemetery-poisoning,—arsenic being the only substance in dispute,—Prof. Reese considers that it is impossible, from the insolubility of that substance and the absence of direct chemical action, that it should be capable of

such transudation. "If the poison was deposited in the organs during life by absorption, the viscera will contain as much of the poison in their interior as on their surface, which would not be the case if they had derived it by post-mortem imbibition. But if the poison is found in the organs of a body after many years' interment, then the objection may be very plausibly urged that its presence in the different viscera may be ascribed to imbibition from the stomach and bowels."

The practitioner must endeavor to cultivate an accurate knowledge of the evidences of poisoning as derived from symptoms, post-mortem examination, chemical analysis, etc. It would be improper, as a general rule, to convict on any one link in the chain, unless some other corroborative link were added. Certainly, so far as symptoms are concerned, the view of Orfila that a case of poisoning can never be proved by them alone is the correct one, inasmuch as there are no characteristic symptoms of any single poison. If it were possible to decide on symptoms alone, as the author remarks, there would be no necessity for a chemical analysis. Although it is possible to establish the fact of poisoning without any chemical analysis, the public at large is not satisfied that justice has been done to or for the prisoner until the refinements of chemical manipulation have been brought to bear upon all matters at issue admitting of such an application. The author contends that the experimenter should never be satisfied with the results obtained from a single test or from a single line of testing. "Thus, in testing for prussic acid, the analyst is not justified in stating, as was done in the Schoeppe case, that this poison was present, simply because 'faint traces' were observed by the application of only two out of the four recognized tests for this substance, and one of these (the iron test) very doubtful."

He states that, in all cases of poisoning by the metallic compounds (arsenic, antimony, mercury, etc.), it is essential to obtain the *metal*, and in such quantity as to enable it to be subjected to all the corroborative tests.

Important as color-tests are, they are frequently fallacious, and the author gives several excellent reasons for the avoidance of a merely mechanical reliance on color alone. Honorable participation in recent medico-legal cases has given Prof. Reese much food for reflection on this point.

"Even the well-known and characteristic color-test of strychnia, which is not imitated by any other known substance, should not be exclusively relied upon in a criminal case, but ought to be corroborated by other tests."

A chapter is devoted to "Compound Poisoning" and the "Antagonism of Poisons," the former having excited popular interest recently from the effort to prove in the Schoeppe case that prussic acid and morphia had both been administered. In regard to the antagonistic action of poisons, as prussic acid and morphia, it is difficult to draw any absolutely conclusive inferences from experiments on the lower animals, in which the toleration for certain agents is sometimes even greater than in man.

Of late years the public has been naturally perplexed in the employment of the expressions *expert* and *expert testimony*. Unfortunately, as usually presented in criminal cases, medico-legal questions become so frequently the texts for quibbling and contention, that in the confusion of discordant testimony they elicit but little sympathy for either of the learned professions interested. An expert should be uniformly "a skilled witness; one who has accurate knowledge of the matter under consideration;" but it must be rather mortifying to one really deserving of such an appellation to find the term bestowed on all those whom the lawyers on either side may so designate, and semi-scientific testimony quoted and discussed as if it had the weight of authority. The whole system does injustice to superior intelligence and culture, when brought in public collision with mediocrity in the court-room.

"If it were only possible," as the author remarks, "to exclude these improvised experts from poison-trials, and confide the responsibility to men of known and recognized toxicological ability, there would occur far fewer occasions in which this conflict of expert-testimony would be witnessed; for it is to be observed that among *genuine* experts—persons of purely scientific attainments, uninfluenced by either prejudice or favor, not mere partisans—this difference of opinion on a purely professional subject is much less likely to occur. In Germany, the experts first summoned are exclusively those whom the State, after proper examination of their competency and skill in such particular inquiries, has duly authorized to act for this purpose; while in addition to this, there is organized a tribunal of experts, to which the opinions of expert witnesses can be referred." We heartily agree with the author that adequate compensation should be given to expert witnesses, commensurate with the high duties they have to perform and the great importance of their testimony.

The greater portion of the work is devoted to poisons proper, their actions, antidotes, tests, etc., embracing all the light which modern investigation has thrown upon the methods of analysis and detection. The classification adopted is modified from that of Taylor.

We cannot do otherwise than commend the writer of this excellent volume for the good taste he has displayed in avoiding personal allusions, such as might very readily have been anticipated from one whose recent testimony in several important criminal cases had brought him into controversy with men of opposite and inflexible views. By thus keeping his work within strictly scientific limits, much more importance is given to the results of his labors. The book, resting on its own merits, on the reputation of its author, and on the importance of its subject, will, we trust, take a well-deserved place as a standard authority on toxicology. In conclusion, we may add that the volume is issued in a handsome and attractive style of typographical execution.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY, FROM JULY 28 TO AUGUST 3, 1874, INCLUSIVE.

BROWN, JOS. R., SURGEON.—Relieved from duty as Medical Director of this Department, having been appointed President of the Army Medical Examining Board, to meet in New York City on August 4, 1874. G. O. 7, Department of the Platte, July 23, 1874.

PAGE, CHARLES, SURGEON.—To act as Medical Director of this Department until the arrival of the medical officer ordered to that duty. G. O. 7, c. s., Department of the Platte.

RANDOLPH, JOHN F., SURGEON.—Granted leave of absence for thirty days. S. O. 104, Department of the Platte, July 29, 1874.

WRIGHT, J. P., SURGEON.—Assigned to duty at Fort Hays, Kansas. S. O. 117, Department of the Missouri, July 30, 1874.

GRAY, C. C., SURGEON.—When relieved by Assistant-Surgeon Middleton, to proceed to Fort Duncan, Texas, for duty at that Post. S. O. 116, Department of Texas, July 28, 1874.

JANEWAY, J. H., ASSISTANT-SURGEON.—When relieved by Surgeon Wright, to proceed to Fort Wallace, Kansas, for duty at that Post. S. O. 117, c. s., Department of the Missouri.

WATERS, W. E., ASSISTANT-SURGEON.—To proceed, without delay, to Fort Dodge, Kansas, and report in person to Colonel N. A. Miles, 5th Infantry, for duty. S. O. 116, Department of the Missouri, July 29, 1874.

MIDDLETON, P., ASSISTANT-SURGEON.—Assigned to duty at Fort Clark, Texas. S. O. 116, c. s., Department of Texas.

KING, J. H. T., ASSISTANT-SURGEON.—Granted leave of absence for ten days. S. O. 149, Military Division of the Atlantic, August 1, 1874.